From the Author

Philosophical Institute of Canterbury,

NEW ZEALAND.

ADDRESS

DELIVERED BY THE PRESIDENT

JULIUS HAAST, PH.D., F.R.S.,

ON

THURSDAY, MARCH 5, 1874.

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Gentlemen.—When two years ago you kindly assented to my request to elect some other member of our Society as your President, I thought that you would continue to do so at least for several years more, hoping that under those circumstances we should have been privileged to listen to a series of addresses for the opening of each session in which the president elect would have given us, either the result of his own individual studies, or the experience obtained during the performance of his professional duties.

However, whatever may have been my individual wishes, I have bowed to the flattering and unanimous opinion of the members of this Society, and have again ascended the presidential chair, trusting that you will kindly overlook my shortcomings. Once more I beg to thank you for this proof of your confidence, and I wish at the same time to assure you that, as in the past, so in the future, it will be my earnest endeavour to advance the interests of our Society, which I trust will rise in a very few years to a conspicuous place amongst its sister institutions in New Zealaud.

Generally, it is the custom of the president elect of a scientific body to devote his opening address either to a general survey of the scientific work done during the year, to allude to important discoveries in the several branche of science, or to select one or several special subjects, of which, by his own vocation, he is able to trace the advancement in years past.

Owing to the peculiar geographical outlines with which New Zealand is endowed, we do not possess one intellectual centre, as is the case in most older countries, or even in many of the neighbouring colonies; but the favourable position and high aspirations of most of the provincial capitals, aided by the foresight and wise legislation of the Provincial Councils, have secured to them peculiar advantages, which, gencrally, are not neglected, and will, I have no doubt, be greatly instrumental in securing the rapid intellectual and material development of this colony in every direction.

Under these circumstances, the President of this Institute, like those of the other societies forming part of the New Zealand Institute, has followed a middle course, and by devoting some portion of his address to general observations, has not neglected to enter into those special topics with which, by original research, he is best acquainted. And whilst the New Zealand Institute has done good work in acting as the publishing medium of these societies, I think, in the interest of the colony, and of members of the affiliated socicties, that a further step should be taken to make this central institution still more useful.

Amongst the improvements which I might venture to suggest, it would simplify matters very much if the Presidents of the five affiliated societies, or as many more as join in the future, were Governors *ex officio*, by which the Board of Governors would gain in strength, and give each society, as it were, a personal interest in the doings of the Central Board, always provided that their attendance and assistance are required, and that their office is not an honorary sinecure.

At the same time it would be desirable to have a general meeting of all the Governors each year at one of the centres of population, giving precedence to those where affiliated societies are located. During these meetings, which might be arranged in the manner of those of the British Association, for the Advancement of Science and similar institutions on the Continent, the principal work of the year could be done, and thus all the chief towns in the colony would in their turn derive the advantages of such meetings. Acquaintances would be formed, to mutual advantage, and local rivalries led, at least in intellectual matters, into such channels, that they would benefit the country at large.

And thus the high position which the New Zealand Institute has already obtained amongst kindred societies would not only be maintained, but the advantages derivable from it would become more manifest in each part of the colony where the meetings of its members should be held.

Proceeding to the few topics I have chosen for to-night, I wish to make first a few observations on the *Geology of the Canterbury* plains, as far as their mode of formation is concerned. I thought that this subject, to which I have devoted considerable time, and of which my reports on the formation of the Canterbury plains, 1864, and on the headwaters of the River Rakaia, 1867, give the necessary data, did not require any more consideration except adding those new details which further surveys and altitude observations, or railway cuttings, &c., would bring within our reach.

However, as Captain Hutton—in a paper "On the date of the last great glacier period in New Zealand, published in the transactions of the New Zealand Institute, vol. V., pages 384 to 393—has come to the conclusion that the Canterbury plains are of marine formation; although, when writing that paper, he had never seen them, and moreover finds in a most peculiar way in my own reports a portion of the proofs for his assertion, I am obliged to return to this subject to put the reader of that article on his guard; the more so, as Captain Hutton, since the article alluded to has been written, paid a flying visit to the Malvern Hills, examining, at the same time, the middle course of the Rakaia and Waimakariri rivers, and, as he since informed me verbally, has not changed his mind in respect to this geological question.

Fortunately, since my reports were written, the extensive surveys of Mr Doyne and other gentlemen made for railway and other purposes have confirmed, in a remarkable degree, my views concerning the "fan" character of the deposits of the principal rivers in every respect. I wish to refer here only to the interesting and highly instructive map attached to Mr Doyne's second report upon the river Waimakariri and the lower plains, where the fan levels are shown over a large area of ground.

Instead of refuting all Captain Hutton's principal arguments, or showing how that gentleman has not read my reports with such care as he should have done if he intended to quote therefrom, I may be allowed to present you, as concisely as possible, with a short *resumé* of the points at issue.

I stated and proved, as I trust, somewhat satisfactorily, that in post-pliocene times—without, however, being obliged to assume greater elevation of the land, which may or may not have existed—glaciers of enormous size were formed, which reached far down the present river valleys, in some instances even advancing beyond the eastern boundaries of the ranges now bordering the Canterbury plains proper. Of these gigantic ice streams, the glacier advancing through the then united valleys of the southern Ashburton and northern Hinds, was, if not the largest, at least equal in size to the Rakaia glacier, owing to the fact that it received enormous additions from the valley of the Rakaia (by the lake Heron) and from that of the Rangitata (by the lakes Tripp and Acland depressions). See "Haast's Report on the Formation of the Canterbury Plains," page 9 and seq. It will thus at once become manifest that Capt. Hutton's argument (page 387) concerning the small size of the present Ashburton and Hinds rivers falls to the ground, and that he was not sufficiently acquainted with all the facts given in that report of mine.

We thus have north of Timaru four distinct

fans, namely, those of the Waimakariri, Rakaia, Ashburton, and Rangitata, with smaller rivers having their sources in the front ranges running between them; the Selwyn between the Waimakariri and Rakaia fans, the Northern Ashburton between those of the Rakaia and of the Ashburton—Northern Hinds; and the Southern Hinds between the latter and the Rangitata fans. The gravel formation of these fans, where they remained undisturbed, does not warp, as Captain Hutton assumes, round the spurs of the hills at the same level that it has at the river gorges, but has a steady fall towards the small streams flowing between the fans of the two large glacier torrents; however, in some instances, this has been concealed by detritus from the mountains, or by re-arrangement of the original river beds on the surface of the upper portion of the plains when the glaciers retreated. But, I may add, the general outlines are nevertheless clear and distinct.

In my geological notes on the Malvern Hills (Reports of Geological Explorations during 1871-72, Wellington, 1872, pages 33-36), I have given an illustration of this. I have shown how the great Rakaia glacier, having also an outlet by the upper course of the river Selwyn, covered with its gravel deposits the lower eminences forming the Malvern Hills, west of the dolerite range, and had its outlet in a N.E. direction in the neighbourhood of Little Racecourse Hill, thus throwing doubtless the bed of the Waimakariri more to the north. When this glacier outlet ceased to flow and to deposit any more boulders and gravel in the district alluded to, the Waimakariri soon began to remove the alluvial beds, thus formed by the Rakaia branch, until harder rocks upon which they were reposing were reached. This fact alone, I trust, will prove that a detailed examination of all physical features in that portion of the country is requisite to enable us to understand the sometimes complicated nature of the fluviatile beds, and that my explanation of the formation of the Canterbury plains is not a mere hypothesis, but based upon a great number of observations made during a number of years.

Captain Hutton, in the same paper, observes: —"It is so universally acknowledged amongst geologists, that river terraces prove elevation that it is quite unnecessary to go over again such well trodden ground," and he brings forward a formidable array of scientific authorities in support. However, nobody ever doubted his statement, but he forgets that there is still another and important agency by which terraces are formed, and which not only in New Zealand, but in many other mountainous regions, has been the principal if not sole cause of their formation, namely, the retreat of the river sources to higher and more distant regions. In my different reports, already cited, I have treated of that subject at length, and shown why and how rivers with less velocity do gradually lower their beds, so that I need not repeat myself here.

But a still more formidable objection to Captain Hutton's hypothesis presents itself:—If the Canterbury plains were of marine origin, the beds of which they are composed would have preserved some traces of it; but although we have clear sections, several hundred feet high, in almost every river, their fluviatile character is unmistakeable. The boulders, shingle, gravel, sand, and ooze are all deposited as a river torrent would place them, according to their form and size, and according to the greater or less amount of water being brought down. The peculiar character of surf shingle is nowhere exhibited, but all the pieces of stone have the subangular form so peculiar to river shingle. Marine fossils are missing throughout.

Moreover, if elevation had taken place during the postpliocene or glacier period, Banks Peninsula would certainly show this most conspicuously, but what does a close examination of that interesting, isolated, volcanic region reveal to us? We observe no trace of marine action, except the results of a slight oscillation of about 20 feet, by which the Peninsula has been raised, after undergoing probably a similar submergence. It is true, that its lower portion, in several localities up to 800 feet, is covered more or less with silt—a fine loam—which in many instances is a true slope deposit, partly derived from the decomposition of the rocks in situ, or partly brought down from higher regions by running water. Moa bones and pieces of small land shells have been found in these deposits, of which there are many splendid sections to be examined, but nowhere the least sign of marine life could be detected in them.

This fact alone shows that the emergence theory has not the least foundation; on the contrary, from the nature of these silt beds and their partial denudation, we might conclude that the Peninsula has undergone a depression since they were deposited. Had a

rise of the ground taken place, by which the Canterbury plains had emerged from the sea, we certainly would find the proofs of it along the slopes of the Peninsula in the form of raised beaches, deposits of sea shingle and sand with recent marine shells, but nowhere is a trace of such easily recognisable beds to be found, and thus, even assuming that the clear and undeniable data which the Canterbury plains present as to their origin were not in existence, the character of the silt deposits on the slopes of Banks Peninsula and the absence of recent marine beds would at once compel us to reject Captain Hutton's new theory as incorrect in all its issues.

Captain Hutton's attempts to prove the correctness of his own views by selecting a few unconnected passages from my own reports, which show, as I believe, clearly the subaërial formation of the Canterbury plains, is rather ingenious; but where he has done so, he has either failed to follow the drift of my reasonings, or he totally misunderstood the explanations I gave of the observed facts.

And with these few remarks I wish to leave the subject, but not without expressing a wish that those who intend to learn something more of the matter should examine for themselves the points at issue, as accurate observations can be made, as it were, close to our doors. Moreover, it is not my intention to refute in detail any theories which are unsupported by facts, as I should have to repeat what I have written before on the subject; and in future I shall only reply with the words, "Go and see," used by Desmarest, one of the fathers of geology, when, towards the end of last century, the Neptunists wanted to draw him into an argument about the nature of basalt.

I have hitherto refrained from publishing any of my notes on the researches made during a number of years upon the accumulated treasures obtained in the turbary deposits of Glenmark, except a list of measurements of leg bones of different species in the first volume of our Transactions, and the description of the bones of the remarkable genus Harpagornis, in Vol. IV., always expecting that Professor Owen, whose truly classical labours have laid the foundations of the edifice to which present and future researches will only form additions, would review himself the whole subjects at length.

Finding, however, that instead of doing so, that illustrious comparative anatomist is inclined to unite, as it were, all the principal species with a struthious character into one genus under the general term of *Dinornis*, dropping altogether the name *Palapteryx*, I feel that I should not do my duty if I were to hold back the following notes any longer.

If it were our good fortune that Professor Owen could have access to the rich material which is exhibited in the Canterbury Museum, I am sure he would never have united under one genus a number of species which show such a remarkable diversity of character; but as his description of single bones of some species, or at most of portions only of others, were given during a considerable space of time, ranging over more than thirty years, I can easily understand that Professor Owen will find every day, as the material increases, greater difficulty to make himself acquainted with all the details, unless he could have such a complete series as we possess in the Canterbury Museum to refer to. Such a series would have afforded him at a glance a confirmation, that the new arrangement which I venture to propose in the following notes, is not based altogether upon unsound principles.

I am well aware, that there are still many naturalists who think that the division of the bones of our extinct avifauna into so many species is a mistake, and that future researches will prove that what appeared to Professor Owen as several well-defined species were after all only various stages of age and growth of one and the same kind. However, in this respect, the collections of the Canterbury Museum bear a strong confirmation of the correctness of the great English anatomist's conclusions. We possess, not only young bones of each species, from the chick to the full-grown bird, where—to take only one bone as guide—the tarsal epiphysis of the metatarsus is not yet quite anchylosed,* but we have of each species a series of specimens of generally two well distinct sizes, from which we may conclude that they represent the male and female bird of each species. In some instances, of which I shall speak more fully in the sequel, we possess of each species four distinct

* We possess, amongst others, the leg bones of a specimen of Dinornis maximus which is in size only second to the largest bones we have, but in which this immature character in the metatarsus is not yet quite effaced.

sizes which might represent the two sexes of two distinct but closely allied species.

Although Professor Owen thinks that the back toe (hallux) was only a small functionless appendage to the foot, and that thus the existence or non-existence of such bone is of no consequence, and has therefore felt obliged to abandon this ground of generic distinction, I am more convinced than ever that it is of great importance, and that the principal division of our extinct struthious birds has to be based upon this, as I believe, constant character.**

If we add to this all the other distinctive features, which I shall enumerate in the sequel, such as the existence or non-existence of a bony scapulo-coracoid, the shape of the sternum and of the bill, and many others, the presence or absence of a hallux becomes of still more importance.

And I might add here another important peculiarity in these two main divisions, which was first pointed out to me by Mr Fuller, and which is of great practical value when examining even the smallest bones. Mr Fuller has found that in the mere handling of the bones a great difference is at once to be detected amongst those coming from the very same spot. Thus the remains of Palapteryx are harder, and have resisted more effectually the influence of time than those of Dinornis; the exterior dense crust is far stronger and thicker, and is less smooth than in the latter. Moreover the bones of the Palapterygidæ are not quite so porous as those of the Dinornithidæ, and consequently are heavier in proportion.

After these few introductory observations I now proceed to lay before you the scheme after which I propose grouping together the different species of our extinct struthious birds, giving at the same time some of the principal distinctive features of each group:—

A. Family Dinornithidæ.

a. Genus Dinornis.

Metatarsus long, no hallux, pelvis narrow, sternum longer than broad, convex, with constant and well marked coracoid depressions for the scapulo—coracoid bone; narrow and straight anterior crest, costal processes slightly developed, lateral processes standing at less angle than in the Palapterygidæ. Existence of a bony scapulo-coracoid; beak narrow and pointed—three intercostals; skeleton altogether of a more slender stature than any of the Palapterygidæ:—

1. *Dinornis maximus.*
2. *Dinornis robustus.*
3. *Dinornis ingens.*
4. *Dinornis struthioides.*
5. *Dinornis gracilis.*

*b. Genus Meionornis.**

Metatarsus long, no hallux, pelvis narrow like Dinornis, and the whole skeleton altogether more slender than any of the Palapterygidæ. Sternum convex, longer than broad, with a broad and well curved anterior border; costal processes well developed, no coracoid depressions; bony scapulo-coracoid absent, beak well pointed, and even narrower than in Dinornis.

1. *Meionornis casuarinus.*
2. *Meionornis didiformis.*

B. Family Palapterygidæ.

a. Genus Palapteryx.

Metatarsus very short and broad, with hallux and hind toe; distal trochleæ remarkably broad and divergent, tibia with both extremities largely developed and standing inward, so as to give the skeleton a bow-legged appearance. Pelvis, very broad and like the bones of the leg, and the rest of a truly pachydermal character; bill very

** I formerly believed that an impression observed on the back of one of the first metatarsals of Dinornis ingens I ever obtained was there for the articulation of the back trochlea, but since then several more specimens of that species have passed through my hands, which showed that impression either only faintly or not at all. Dr Jaeger, of Vienna, articulated a small back trochlea with the skeleton of Dinornis ingens found in the Moa cave of Nelson, but there is no evidence that the small bone in question belonged to it. In my first paper of measurements on page 85 of the first volume of the Transactions of the New Zealand Institute, I already pointed to the distinct rough groove which *invariably* exists at the back of the metatarsus of a number of species, which I have now ventured to unite under the term *Palapterygidæ*. I may add that a number of back trochleæ in the possession of the Canterbury Museum as to form and size, agree in a remarkable degree with the form and size of the bones of the different species belonging to that family. It would be strange if this striking coincidence, together with the rough grooves previously alluded to, would have misled me to draw wrong conclusions therefrom.

* From *meion*, less; and *ornis*, bird.

obtuse and rounded at the tip; sternum flattened, broader than long, with a strong costal process, lateral processes standing at a higher angle than in any of the Dinornithidæ; no coracoid depressions in aged specimens; no bony scapulo-coracoid, two intercostals only.

1. *Palapteryx elephantopus.*
2. *Palapteryx crassus.*

b. Genus Euryapteryx†

Metatarsus short and broad, but not so pachydermal as the former, with a hallux and hind toe; tibia, straighter, and without the extremities so enlarged as in Palapteryx; sternum longer than broad, more concave than the former genus, without coracoid depressions, but with strong and long costal processes, mesial portion and process comparatively longer than in all the former subdivisions, no bony scapulo-coracoid, beak not so obtuse as in the former.

1. *Euryapteryx gravis.*
2. *Euryapteryx rheides.*

In the preceding list I have only entered those well-defined species of which we possess ample material for comparison and generalisation, leaving several others, of which we obtained only portions, for a future notice; but amongst them I may at least allude to one species which appears to approach the Emu of Australia in its general characteristics.

I had also the intention to add some notes on the crania of the different genera, but fear that it would make this address too long were I to give them here.

However, before proceeding, there is one point to which I wish to draw your attention, namely, to the existence or absence of a bony scapulo-coracoid. In the genus Dinornis we find deep and well-defined coracoid depressions in the anterior border of the sternum of each species; and the excavations have furnished us with a series of scapulo-coracoids, which fit exactly into these depressions. Moreover, these small and peculiar bones by their form and size agree also in other respects well with the different species enumerated. However, when we examine the sternums of the genus Palapteryx, and principally that of Palapteryx elephantopus, we meet some with well-marked depressions, others with only faint ones; whilst there are others belonging apparently to aged birds, where there is not the least appearance of them. Again, we possess a few sternums in which a depression exists on the one side, whilst it is missing on the other; so that we are compelled to conclude that no bony scapulo-coracoid could articulate with them.

Moreover, we have never found any scapulo-coracoids of a different form from those articulating with the five species of Dinornis, and as we have obtained a number of the most minute bones of the smallest species, it would be difficult to conceive that a bone of such considerable size should altogether have escaped; the more so, as so many specimens of Palapteryx were excavated. And, although this is only negative evidence, it is so strong that there is not the least doubt in my mind of the non-existence of a bony scapulo-coracoid. The same might indeed have existed in a cartilaginous form, attached to the sternum by cartilage, but of this we have no evidence. I am well aware that on physiological grounds the presence of that bone seems to be indispensable for the mechanism of respiration in birds, as Professor Owen has shown from his dissection of Apteryx, and he has lately again called my attention to the fact (letter to me, dated British Museum, Aug. 5, 1873), but, with the data at present before us, I cannot alter my views, the more so as I do not deny that such a process might have existed as cartilage.

It will be seen from the subdivisions given above that I have not used the term Dinornis giganteus, as there seems to be a specific difference between the species of that name from the Northern island to which that term was first given by Professor Owen, and the largest bird of this island. In this I have followed Professor Owen, who has proposed the specific term of Dinornis maximus for the latter, which appears to have been altogether of more gigantic proportions than the Northern Island bird. I was once under the impression that a specific difference could be traced between the largest skeletons known, for which the above term *maximus* was first used by Professor Owen, and the somewhat smaller skeletons, for which for some time the designation *giganteus* was retained by me; but after a careful examination of a number of skeletons, there remains not the least doubt in my mind, that they belong all to the same species, with a graduai decrease of size and robustness.

And even assuming that the largest skele-

† From *eurys* broad, and *apteryx* without wing.

tons belonged to the female birds, a similar considerable difference in size being also constant with the different species of Apteryx, there are so many intermediate forms, that even the supposed line of division between both sexes is exceedingly difficult to draw. Moreover, and this is peculiar to *Dinornis maximus*, there are scarcely two skeletons entirely alike; there are some which have a remarkably long metatarsus, whilst the other leg bones do not (at least at the same rate) increase in size; others are much stouter for their height. Altogether we might trace the same peculiarity in size and form as in a series of human skeletons, selected at random.

The same is the case with the skeletons of the immature birds of this species, of which we possess portions from the chick to the full-grown giant bird, where the tarsal epiphysis is not yet so closely united with the metatarsus, that the line of junction is still visible, where also a similar variety of form can be traced.

The difference in size between *Dinornis maximus* and *Dinornis robustus*, the next in size is very marked and constant. Of the latter we obtained a series of two sizes, of which the largest might be assigned to the female.

Between *Dinornis robustus*, *ingens*, *gracilis*, and *struthioides*, besides their well defined specific characters there are also distinct breaks; each species possessing at the same time two constant sizes.

Of *Meionornis casuarinus* a series of four clearly defined sizes are in our possession, so that we might conclude, that we have two closely allied species before us, of which the two largest sizes represent male and female of the one and the two smaller male and female of the other.

A considerable difference in size occurs between the smallest species of *Meionornis casuarinus* and the largest species of *Meionornis didiformis*. In the latter we can distinguish also four sizes with a gradation similar to that observed in the former, so that I am led to believe, that this species, like *Meionornis casuarinus*, consists of two subspecies.

If we compare two skeletons of Apteryx Australis, male and female, and two of Apteryx Owenii, male and female, with each other, a similar distinct gradation is observable.

Palapteryx elephantopus has also four well distinguishable subdivisions, of which the largest size is the most conspicuous and best marked, so that the suggestion ventured concerning two sub-species belonging to *Meionornis casuarinus* and *didiformis* applies equally to this remarkable extinct bird.

The division between this and the next species *Palapteryx crassus* is well marked, consisting, moreover, of two constantly maintained sizes.

Euryapteryx gravis and rheides, which can easily be distinguished at a glance, from each other, not only by their size, but by their anatomical characteristics, consist each of two sizes only, and, as I suppose, to be attributed likewise to difference of sex.

Amongst other species of extinct birds of which the Glenmark turbary deposits have yielded remains, there is first the huge diurnal bird of prey, which I described under the specific term of *Harpagornis Moorei*. Another remarkable species is a rallinc form of gigantic size, *Aptornis*, of which we obtained sufficient material for articulation, and which is closely allied to Ocydromus, the woodhen.

The remains of *Cnemiornis*—a gigantic goose, as first pointed out by Dr Hector—have hitherto been very scarce, so that we possess only a few bones of it. It is remarkable that the excavations, undertaken during a number of years, did not yield a single bone of *Notornis*, which therefore did either not inhabit this part of the country or was of extremely rare occurrence.

Of other species we obtained bones of Apteryx, Strigops, Ocydromus, Himantopus, Botaurus, Hæmatopus, several species of ducks, and of a number of still smaller birds, which cannot be distinguished from bones belonging to recent species. The remarkable fringed lizard, Hatteria punctata was also an inhabitant of this Island as several bones belonging to it were found with the Moa bones.

Professor Owen having described at some length in several of his memoirs on Dinornis, the affinities our struthious birds bear with those of other countries, pointing out at the same time, the peculiarities through which they vary from them, it would have been unnecessary for me to add anything to the subject, had not lately the attempt been made by Professor Alphonse Milne-Edwards, in Paris, to show from a comparison of the remains of the extinct ornithic fauna exhumed in Madagascar, Mauritius, and Rodriguez, that in some distant ages New Zealand formed portion of a large continent or of a group of more or less extensive islands in the Southern hemisphere,

which at one time were in some way connected with each other.

He thinks that additional confirmation can be obtained from the ascertained occurrence of different Ocydromidæ, such as the Aphanap eryx and the Miserythrus Leguati, which latter, he informs me (letter to me, dated "Jardin des Plantes, Paris, Aug. 3, 1873), bears close resemblance to our common woodhen (Ocydromus Australis).

However enticing the tracing of close affinities must be to the naturalist-philosopher, I believe, that it would be rather rash to conclude the connection of two such distant insular groups from a few forms of birds only. Leaving the general question alone for the present, to which I shall return shortly, it is impossible for me to conceive that two countries, which in all other respects have such a dissimilar and distinctive flora and fauna could have been united in any way without having left other living proofs of such connection in their present endemic organic life, not to speak of fossil remains.

We know that Madagascar is a zoological sub-province of South Africa (Ethiopian region), but having a fauna so peculiar, that it must have, according to Sir Charles Lyell, been separated from Africa probably since the Upper Miocene era.

New Zealand, on the other hand, although it may have been formerly of larger extent, has never been more than an oceanic continental island from a zoological point of view, a theory first propounded by Darwin and Wallace, and with which I fully agree.

It would be rather a difficult task to prove upon such slender grounds as the presence of a few species of struthious and ralline birds will afford, that both countries could possibly have been connected. Moreover, the difference in the anatomical structure of the three Madagascar species of *Aepyornis* and of the New Zealand *Dinornithidæ* — using this latter term in a general sense—is so enormous that I fail to see how they possibly could prove that connection in any way.

I cannot agree with Professor Alphonse Milne Edwards, that the Aepyornis stands nearer to Dinornis than to the Ostriches, Casuaries and Emus, except that the fossil bones of Madagascar and New Zealand have a more pachydermal type, than the recent species named. But I may point out, that the fossil Dromornis Australis of Australia shows similar characteristics, and I am sure if fossil remains of struthious birds in beds of postpliocene age were discovered in Africa, America, and Asia, that they would exhibit a similar pachydermal character.

Judging from Professor Milne-Edwards' own excellent memoirs on Aepyornis and the fine casts of the unique fossil bones in the Paris Museum, he was good enough to send to the Canterbury Museum, I am unable to trace their relationship with our Diuornithidæ. It appears to me that the Madagascar species are separated from the former by many fundamental differences, such as (to point out only a few) the pneumatic foramen in the femur and the straightness of the trochleæ of the metatarsus.

And although I am convinced that the struthious character of Aepyornis has sufficiently been proved by the eminent Paris comparative anatomist, I can easily understand that there was at first some show of reason for placing it amongst the sarcoramphous vultures, as has been done by Professor Bianconi.

However, speaking of the principle itself, I wish to point out, if we were to decide from a few isolated species in two distant countries which show some or even a close resemblance to each other, that these countries must have ouce been conuected in some way, we would in many iustances form erroneous conclusions. We might as well say, that because there are struthious birds in Australia, the Malay Archipelago, Africa, America, and Asia, all these countries must have been connected with New Zealand, or because marsupial remains have been found in secondary rocks in Europe and several species of opossums are living in America, these countries had also been united with Australia.

Speaking from a general point of view, I wish to add that the attempts to trace the geographical relations of a fauna and flora of a couutry can easily be exaggerated, and thus a theory be ridden to death which otherwise would be very useful.

Moreover, an unfortunate country, such as New Zealand, of which a good number of the species of its fauna and flora show great resemblance with other species from distant countries, has to be dipped down and brought up again a great many times, in order to establish connections in various directions, so that a bird or fish, a shell, insect, or centipede might cross from the one to the other,

moreover, without allowing any other species from the same country to pass.

Besides, the geological record of these islands at present at our disposal does not warrant us to assume such repeated changes in the level of the land.

Can the explanation of such close specific resemblance not be found, in many instances at least, in the adoption of more simple natural causes, such as the transport by icebergs, or on floating islands, by birds, etc., and of which Sir Chas. Lyell, in his great work, the "Principles of Geology," gives many striking instances?

However, where the theory of land connection is not admissible, and where also others, which have hitherto been applied, fail, might we not assume that similar climatic and other physical conditions could produce similar specific characters under the great law of evolution?

It is a most difficult problem to say what constitutes a species, and therefore might it not be safer to believe until the impossibility of such a hypothesis has been demonstrated satisfactorily, that there exists a *similitude* as well as an *identity* of species under certain given conditions.

In one word, might we not throw out the conjecture that in two more or less distant countries whi h never were directly united, some forms of organic life can and do exist, which show what to us appears identical specific characters, because the cause or causes of their evolution were identical or nearly identical, and thus a considerable number of supposed changes in the level of many countries of which we do not find geological records, can be dispensed with.

It is true, that instances to be explained by the migration or accident theories are of more frequent occurrence and more easily proved, but I think it would be just as interesting, where these cannot be admitted, to trace in all its bearings the similitude of species in distant countries. This view would, at least, open up a field of fresh research, and afford a new illustration and confirmation of the great theory of evolution.

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